Reply to PCIRR RNR2 decision letter reviews:
Rozin et al. (1997) RRR

We would like to thank the editor and the reviewers for the suggestions The editor's and reviewers' comments are in bold with our reply underneath in normal script.

A track-changes comparison of the previous submission and the revised submission can be found on: https://draftable.com/compare/LXqpRAZyDMfS (https://osf.io/psyvc)

A track-changes manuscript is provided with the file: PCIRR-S1-RNR2-Rozin-etal-1997-replication-manuscript-v1-track-changes.docx (https://osf.io/nb948)

Reply to Editor: Dr./Prof. Michèle B. Nuijten

> **Thank you for submitting a revised version of your manuscript and for your detailed replies to the reviewer comments. The reviewers that looked at your first submission had kindly agreed to also review your revision. As you can see, they are happy with the changes you made and think that this study would be a valuable contribution to the literature.**

Thank you for the reviews obtained, your feedback, and the invitation to revise and resubmit.

> **The only remaining issue that all reviewers mention in different ways is a question of how this replication study fits into the broader literature. This is related to criteria 1A and 1B of PCI:RR and also ties in with my previous comments of what the main goal of this replication is.**
>
> **In the reply to the reviewers, you mention that this replication study is part of a large-scale replication project. I think this is relevant information that contextualizes your study and explains some of the choices you make. Specifically, it may explain why several of your choices are more "mechanical" (as reviewer Willem Sleegers phrases it), rather than theoretical. It explains why you do not intend to provide a more in-depth literature review and future research agenda (RE Ben de Groeve), and why you intend to stay close to the effects found in the original study instead of formulating more theoretically informed smallest effect sizes of interest (RE Seth Green). It also helps me understand why you wish to formulate an objective measure to dichotomize replication success for the study as a whole.**
>
> **An in depth discussion of the usefulness and interpretation of different types of replication is beyond the scope of this project, and I agree with you that it is a good thing to simply have more direct replications in the literature. However, if you intend to stick to (potentially suboptimal) choices of the original design of an arguably quite old study, instead of going for a more in depth analysis that could arguably inform theory better, this needs to be justified. I think the fact that this study is part of a larger replication effort would be a good justification.**

> **I would like to invite you to add a few sentences to explain this context to the Stage 1 report, if possible. No further revisions are needed.**

Thank you, we appreciate the suggestion to link to the broader team project.

We added the following as a second paragraph to the “Choice of target article for replication: Rozin et al. (1997)” section:

> This project is part of a mass replications project by the CORE Team (2025) aiming to systematically conduct replications of classic findings in social psychology and decision-making (e.g., Chan & Feldman, 2025; Chan et al., 2025; Zhu & Feldman, 2025). We aim to contribute to the growing recognition of the importance of reproducibility and replicability in psychological science (e.g., Nosek et al., 2022; Zwaan et al., 2018) and of the use of Registered Reports in improving rigor and quality and in combating review and publication biases (Chambers & Tzavella, 2022; Soderberg et al., 2021). Given its impact on the literature, we consider Rozin et al. (1997) to be a seminal finding in social psychology, and believe that stakeholders would greatly benefit from updated evidence of an independent well-powered bias-controlled replication (through a Registered Report) adhering to current best practices of open-science.

Reply to Reviewer #1: Dr./Prof. Ben De Groeve

> **The authors changed their plans based on reviewer comments: they decided to focus more closely on the replication and leave extensions for future research. I think this decision is understandable. However, given the wealth of relevant research published over the last 25+ years or so, I would strongly encourage the authors that they also use this replication as an opportunity to clearly outline a future research agenda (based on their findings and this broader literature). I appreciate that they already plan to discuss some of our suggestions.**
>
> **The authors and reviewers have also identified several limitations in the original study. While one might argue that these limitations weaken the value of a close replication, the authors have revised their study design to better account for them. This enhances the potential for self-correction and significantly improves the replication effort. I look forward to seeing the results.**

Thank you for the positive and supportive note.

Yes, this is indeed the plan. Registered Report Stage 1 is not the best time to include those, yet we indeed plan to expand on that in our Stage 2 once we have our findings. To address your request, as with the other suggestions regarding things to discuss about the target article and the literature, we included a planned discussion in our revised Discussion:

> [Planned discussion for Stage 2: We plan to outline a future research agenda, and position our findings in context of the broader literature.]

Reply to Reviewer #2: Dr./Prof. Willem Sleegers

> **I previously reviewed this submission and left several comments. The authors have responded to my comments and adapted the manuscript in multiple ways to address them. In summary, I think my comments have been sufficiently addressed, although I am left with some concerns still.**

Thank you for the supportive opening note.

> **One of my major comments was that the value of the target article, and why it should be replicated, was not made clear. The author's response is mostly one of arguing for the importance of replications in general and saying that it is beyond the scope of the project to argue for the importance of the target article. I'll defer to the editor whether or not this response is sufficient.**
>
> **I do want to repeat that I do think it is a significant limitation of the proposed study to not more clearly establish the importance of the target article and, in turn, the proposed study. I also think not engaging in this question is a symptom of a more general approach the authors take that I am skeptical of. The authors seem to take a rather mechanical approach to conducting this study. By this I mean they aim to follow a set of rules rather than make decisions regarding the more complex aspects of theory, study design, and data interpretation (as also seen in my comment and their reply on counting successfully replicated hypotheses to determine whether the study as a whole is a successful replication). I think it cannot be avoided that one has to make subjective decisions in doing science and I'd rather see attempts to do so based on argumentation rather than procedure. I can see, however, that this is an approach that might be favored by others and if it falls within the guidelines of PCI-RR, then this seems fine with me.**

We appreciate you sharing your views on this. There is much to discuss and debate on the topic, we have a lot more to share from our extensive experience with replications. We would have liked to hear more about your concerns and skepticism, and then to see exemplars of replication projects that you consider to do a better job at addressing these issues, so that we can compare objective measures that would help assess contributions to theory, methods, design, analysis, interpretability, and generalizability. We feel that it is important for the scientific community to discuss these views and to try and come to some understanding and hopefully objective assessment of what works best and when. Should anyone be interested - we would be very happy to further discuss these as a separate project.

Reply to Reviewer #3: Dr./Prof. Seth Green

> **The revised manuscript looks much improved, especially in its broader theoretical motivation. In particular I like the paragraph beginning with "Beyond the academic attention it received..." I also think the summary of the target article's methods is much improved.**
>
> **I continue to think that defining and motivating the smallest effect size of interest would be of interest to readers. Willem made the same point in his initial review. It's up to the editors whether this is a showstopper or not.**
>
> **No further comments.**

Thank you for the feedback. Similarly to how we responded to Dr./Prof. Willem Sleegers, there is much more to share and discuss, also on the topic of defining the smallest effect of interest, and we would welcome the opportunity to continue such discussions as a separate project.